PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Obesity and sickness absence – Results from a longitudinal nationally representative sample from Germany
AUTHORS	Reber, Katrin; König, Hans-Helmut; Hajek, André

VERSION 1 – REVIEW

REVIEWER	Laura Serra Saurina Center for Research in Occupational Health (CiSAL) Universitat Pompeu Fabra (UPF), Barcelona IMIM (Hospital del Mar Reseach Institute), Barcelona
REVIEW RETURNED	07-Nov-2017

GENERAL COMMENTS	I think that the paper is well written and the content is interesting and the results could be very useful for the scientific community. However, from my point of view, the paper needs a reorganization of some parts as well as further explanation of some others. I add some suggestions to the authors and some minor changes in order to improve or clarify the research done. General Comments 1. The first doubt I have when reading the manuscript is about the design of the study. If I understand properly the study the BMI is recorded in a time period after the outcome as the sick leave days and the long-term absenteeism are quantified retrospectively. I think that a better explanation is needed in order to address the outcome retrospective. In this line, how do you cope with reverse causality? 2. Introduction. In my opinion the last sentence of this section would be more suitable in the discussion section. 3. Study population and methods. 3.1. It would be interesting to specify the total number of the sample in this sample. I would suggest to restructure this part with some of the sample explanation in page 8. This would make clearer the manuscript. 3.2. Regarding independent variables, what does "disability assessed" contribute? I consider that "individuals' self-rated health" it is a good indicator by itself and it would be enough. However, could you explain the contribution of that variable? In addition, life satisfaction is not a psychological factor. It is a quality of life related factor. I suggest to clarify its contribution. 4. Statistical analysis. I would like the authors to clarify whether the dependent variable "sick leave days" may include zero or if the study considers only individuals with SA. If this was the case, it might be interesting to consider using a ZIP model instead of the Poisson

	model. 5. Results. 5.1. In the explanation of table 1 I suggest highlighting only those values that indicate differences between the two samples. In this line I would reduce the second paragraph of the results section. 5.2. I would like a further explanation about the statistical model. How life satisfaction is considered into the model? Is it included as a categorical variable? Is it a continuous variable? I suggest to expand the explanation. 6. Discussion. In my opinion, this section should be reorganized, mainly the part related to limitations. Although most of them are exposed, I would suggest to expand the explanation, especially on the record bias. Specific comments - I would suggest a more concrete title. Page 5. There is a typo error SOEP should be GSOEP Page 6. It is a bit confusing to me how the marital status is dichotomized. The first part is clear to me "married, living together coded as one and zero otherwise", however the second part "(married, living separated from spouse; divorced; widowed; single)" it is unclear. I would suggest to write make this sentence clearer. Page 16. I would change "weight change" by BMI change. All in all I think the paper would need to cover these points (or at least some of them) to be ready for the readership of the BMJ Open.

REVIEWER	Maarit Piirtola University of Helsinki, Finland
REVIEW RETURNED	28-Nov-2017

GENERAL COMMENTS	This study aimed at investigating the longitudinal association between obesity and sickness absence in women and men in Germany. I find the aim of the study important. In common, the study is well designed and reported. However, there are small inaccuracies in the manuscript that should be corrected/ added. Also, the manuscript should be language checked by an expert in English. Comments for the issues to be rechecked and/or rewritten Abstract - Methods: I would like the authors to mention that based on the significant interaction between sex and BMI categories on sickness absence, all analyses were stratified by sex. - Conclusion: First, could the authors state in their conclusion that obesity was associated with higher likelihood for sick leave and long-term absenteeism in women (not just that there was an association). Secondly, actually this study has not studied if weight management would be beneficial in reducing sickness absence. Maybe it would be safer to either exclude the last sentence from the conclusion or to conclude that obese women might need special efforts in aiming to reduce sickness absence at work. Strengths and limitations of the study - could a person expert in statistics check the following sentences: o “Panel regression models were used, diminishing the

	problem of unobserved heterogeneity." and "Attrition bias might be rather small in the current study." (This study uses a panel data in which some of the individuals have taken part all of the surveys and some only parts of the surveys. I think the statement is right for the first sentence but I wonder if attrition bias is small in this study.) In common - I would recommend the authors to use term sex instead of gender throughout the text. - For the readability, please define overweight, obesity etc in the beginning of the instruction and then use the categories without numbers systematically. - Even longitudinal studies (using panel data or not) can define "causality" between the factors. Please use term "associations" throughout the text. Introduction - Please, add a reference to a sentence (page 4, rows 44-46) - Page 5: "This knowledge is important, as effective interventions to treat excess weight might also be fruitful to reduce sickness absence". How do this study actually analyze the effectiveness of interventions? I think you could rather state that association of obesity in sickness absence is important for implementing actions to tackle the problem. Further, the association of sex difference in the association is important. Study population and methods - In general this section is clear and well written. - Maybe I have missed it but some of the participants have taken part in all of the surveys and some occasionally? It would be informative to write this down it the methods. Are those included in the sick leave data also included in long-term absenteeism or have you stratified by the length of the sick leave? - Statistical analyses: Would it be more simple just to stratify the analyses by sex (as you have done) rather than report everything for all, by sex and then even with an interaction term included? I suggest the author the report that they tested the interaction between sex and BMI classes for sickness absence. The interactions were significant and therefore they have stratified their analyses by sex (justifying why the stratification by sex is done). Then stop telling that the interaction was significant between the sexes in "every other sentences". Results - Related to my earlier comments, I suggest that all analyses should be stratified by sex and also reported by sex -> changes to the text of results and tables needs to done. - In general, authors are repeating the tables in their text. I think the authors are trying to help a reader to follow their tables by adding what information they are describing in which columns but this makes the text rather heavy to read. For example, the authors could exclude from the "Regression analyses" sentence from the page 10 rows 7-10 (first column….) and exclude same kinds of sentences throughout the result section. - - "depicted" -> described? (page 8, row 19) - Could you add the total number of observation also to table 1 (under sick leave days and long-term absenteeism)? - In sum (page 8, row 32 ->) are you talking in total or at baseline? - Tables 2 and 3, exclude columns 1 and 4, add information from the foot note (Poisson …. or Odds Ration…) to the headings to help readability of the tables. I also wonder if it would be more

	informative to report 95% CIs instead of SEs? Exclude information related to parenthesis from the headings (first column…..etc). Exclude information related to interaction terms from the tables and add this information to the results or the methods. - Sensitivity analyses were very clear and nicely written, thank you for it. Discussion In general the discussion was logic and informative. I missed some deeper discussion about sex difference related to sick leave. I wonder if the psychological/ psychosocial factors explain the difference. What kinds of factors happen in a woman body when her fat level increases? Are they more prone to musculoskeletal diseases, diabetes or depression? Is obesity a proxy of something else? Small thinks: - Please, add a reference(s) to page 15 row 31. - Page 15 paragraph 4: you mean in Germany? Ethical approval - Please exclude the first sentence.

REVIEWER	Hung-Yi Chuang Kaohsiung Medical University, Taiwan
REVIEW RETURNED	29-Dec-2017

GENERAL COMMENTS	Since the research was a longitudinal study, using 6 repeated (biannual) measurements from 2002 to 2012. The authors used fixed methods that should be not complete. I strongly suggest to use mixed methods. There are many software packages (including freeware) for this kind of statistics.

VERSION 1 – AUTHOR RESPONSE

RESPONSES TO COMMENTS OF REVIEWERS

Comments of reviewers	Response (citations from manuscript printed in italics, changes are underlined)
Editorial Requirements	
Please revise your title to include the location. This is the preferred format for the journal.	Thank you for your comment. We revised our title accordingly: *Obesity and sickness absence – Results from a longitudinal nationally representative sample <u>from Germany</u>*

- Please provide more information about the data used in the study, for example, is this publicly available?	Thank you for your comment. We added further details: *We used data from the German Socio-Economic Panel Study (GSOEP), a representative longitudinal survey of the German population conducted on an annual basis since 1984.[12] The GSOEP is located at the German Institute for Economic Research (DIW Berlin). It is a household panel like the Panel Study of Income Dynamics in the US (PSID) or the British Household Panel Study (BHPS). Every year, approximately 11,000 households and more than 20,000 individuals were interviewed. All adult household members (aged 17 and over) are interviewed.* Please see the „DATA SHARING STATEMENT" section for further details.
Reviewer #1	
I think that the paper is well written and the content is interesting and the results could be very useful for the scientific community. However, from my point of view, the paper needs a reorganization of some parts as well as further explanation of some others. I add some suggestions to the authors and some minor changes in order to improve or clarify the	First, thank you very much to review this manuscript. We really appreciate your comments.

research done.	
General Comments 1. The first doubt I have when reading the manuscript is about the design of the study. If I understand properly the study the BMI is recorded in a time period after the outcome as the sick leave days and the long-term absenteeism are quantified retrospectively. I think that a better explanation is needed in order to address the outcome retrospective. In this line, how do you cope with reverse causality?	Thank you for your comment. We extended the limitations section accordingly: *In addition, long-term absenteeism and sick leave days were quantified retrospectively. Hence, we cannot rule out that the outcome measures affects BMI change (endogeneity bias). Thus, future studies (e.g., based on panel instrumental variable procedures) are needed to overcome these problems.*
2. Introduction. In my opinion the last sentence of this section would be more suitable in the discussion section.	Thank you for your comment. Following your suggestion, the last sentence of this section: *"This knowledge is important, as effective interventions to treat excess weight might also be fruitful to reduce sickness absence."* was removed from the Introduction. We added this sentence to the discussion section with slight changes (please see the suggestion of reviewer #2): *Knowledge regarding the longitudinal association between obesity and sickness absence (and the moderating role of sex) is important for*

	implementing strategies to tackle this problem.
3. Study population and methods. 3.1. It would be interesting to specify the total number of the sample in this sample. I would suggest to restructure this part with some of the sample explanation in page 8. This would make clearer the manuscript.	*In the current study, the analyses were based on data from six waves (2002-2012, bi-annually) because BMI was assessed only bi-annually. We restricted our sample to individuals aged 17 to 65 years, who were in the labor force and employed at all waves. Thus, while regression analysis with sick leave days as outcome measure is based on 48,865 observations, the regression analysis with long-term absenteeism as outcome measure is based on 9,564 observations.*
3.2. Regarding independent variables, what does "disability assessed" contribute? I consider that "individuals' self-rated health" it is a good indicator by itself and it would be enough. However, could you explain the contribution of that variable?	Thank you for your comment. We clarified it: *Concerning health-related and subjective well-being factors, we included* ***subjective*** *health which was based on individuals' self-rated health (5-point Likert scale: 1="bad" and 5="very good") and disability assessed by a single item asking whether they were "legally classified as handicapped or capable of gainful employment only to a reduced extent due to medical reasons" (no/yes). The disability variable served as a proxy measure for* ***objective*** *morbidity.*[20 21]
In addition, life satisfaction is not a psychological factor. It is a quality of life related factor. I suggest to clarify its contribution.	Thank you for your comment. We changed it accordingly throughout the manuscript. For example (page 7): *Concerning health-related and subjective well-being factors, […]* Furthermore, life satisfaction was added as an explanatory variable because it has been demonstrated that life satisfaction/subjective well-

	being is associated with sickness absence. We adjusted the sentence and added a reference: *In accordance with prior research [22], the continuous variable satisfaction with life evaluated by the question "How satisfied are you with your life, all things considered?" (11-point rating scale ranging from 0 "completely dissatisfied" to 10 "completely satisfied") was included.*
4. Statistical analysis. I would like the authors to clarify whether the dependent variable "sick leave days" may include zero or if the study considers only individuals with SA. If this was the case, it might be interesting to consider using a ZIP model instead of the Poisson model.	Thank you for your comment. The Hausman-test performed substantiated our choice (use of FE regression models). Since, to the best of our knowledge, FE zero-inflated regression models do not exist, we would prefer to leave this choice unchanged.
5. Results. 5.1. In the explanation of table 1 I suggest highlighting only those values that indicate differences between the two samples.	Thank you for your comment. However, as we consider both outcomes – sick leave days and long-term absenteeism – separately, we also opted to report sample characteristics for each of the samples (the sample with sick leave days and the sample with long-term absenteeism as outcome) separately: *In total (Table 1, columns 1 and 2), nearly one-half were female (47.8% in the sample with sick leave days as outcome, 48.7% in the sample with long-term absenteeism as outcome). The mean age was 41.9 (±11.2 years; 17-64 years) and 45.4 (±10.4 years; 17-64 years) in the sick leave days sample and in the long-term absenteeism sample, respectively. ~~Roughly two out of three (61.4%) were married, living together with spouse. Mean self-rated health equaled 2.5 (±0.8) and 93.4% were not severely disabled.~~*

	~~*The mean life satisfaction score was 7.1 (±1.6).*~~ *According to the WHO categories, 1.8% were classified as underweight, 48.1% as normal weight, 35.5% as overweight, and 14.6% as obese, respectively.* Examining differences between the two samples was not within the scope of this study, therefore we prefer not to report differences explicitly.
In this line I would reduce the second paragraph of the results section.	Thank you for your comment. *In total (Table 1, columns 1 and 2), nearly one-half were female (47.8% in the sample with sick leave days as outcome, 48.7% in the sample with long-term absenteeism as outcome). The mean age was 41.9 (±11.2 years; 17-64 years) and 45.4 (±10.4 years; 17-64 years) in the sick leave days sample and in the long-term absenteeism sample, respectively.* ~~*Roughly two out of three (61.4%) were married, living together with spouse. Mean self-rated health equaled 2.5 (±0.8) and 93.4% were not severely disabled. The mean life satisfaction score was 7.1 (±1.6).*~~
5.2. I would like a further explanation about the statistical model. How life satisfaction is considered into the model? Is it included as a categorical variable? Is it a continuous variable? I suggest to expand the explanation.	Thank you for your comment. We clarified it in the methods section: *In addition, the continuous variable satisfaction with life evaluated by the question "How satisfied are you with your life, all things considered?" (11-point rating scale ranging from 0 "completely dissatisfied" to 10 "completely satisfied") was included.*
6. Discussion. In my opinion, this section should be reorganized, mainly the part related to limitations. Although	Thank you for your comment. Following your suggestion, we extended the limitations section: *As regards sick leave days, we cannot dismiss the possibility of a recall*

most of them are exposed, I would suggest to expand the explanation, especially on the record bias.	*bias. However, it has been shown that self-reported sick leave can be employed as a proxy measure when administrative data are not available.*[40]
Specific comments - I would suggest a more concrete title.	Thank you for your comment. Following the suggestion of the editor, we added the location of this study: *Obesity and sickness absence – Results from a longitudinal nationally representative sample from Germany*
Page 5. There is a typo error SOEP should be GSOEP	We corrected it accordingly: *We used data from the German Socio-Economic Panel Study (GSOEP), a representative longitudinal survey of the German population conducted on an annual basis since 1984*
Page 6. It is a bit confusing to me how the marital status is dichotomized. The first part is clear to me "married, living together coded as one and zero otherwise", however the second part "(married, living separated from spouse; divorced; widowed; single)" it is unclear. I would suggest to write make this sentence clearer.	*As regards sociodemographic characteristics, we considered age, gender and marital status, the latter being dichotomized with married, living together coded as one and zero otherwise (i.e., married, but living separated from spouse; divorced; widowed; single are coded as zero).*
Page 16. I would change "weight change" by BMI	We changed it accordingly: *Hence, we cannot rule out that the outcome measures affects BMI change*

change.	*(endogeneity bias).*
All in all I think the paper would need to cover these points (or at least some of them) to be ready for the readership of the BMJ Open.	Again, thank you for your helpful comments. It helps to improve the quality of the manuscript.
Reviewer #2	
This study aimed at investigating the longitudinal association between obesity and sickness absence in women and men in Germany. I find the aim of the study important. In common, the study is well designed and reported. However, there are small inaccuracies in the manuscript that should be corrected/ added.	First, thank you very much to review this manuscript. We really appreciate your comments.
Also, the manuscript should be language checked by an expert in English.	Thank you for your comment. A native speaker did proofread the manuscript and corrected language errors throughout the manuscript.
Abstract: Methods: I would like the authors to mention that based on the significant interaction between sex and BMI categories on sickness	Thank you for your comment. We acknowledge the fact that the steps in statistical analysis vary across disciplines. Our approach (which is common in economics and social sciences) was to run the analysis (i) for the whole sample, (ii) stratified by

absence, all analyses were stratified by sex.	sex and (iii) to test whether the sex differences were significant. We clarified it in the methods section (abstract): *Fixed effects (FE) regression models were used <u>for the total sample and stratified by sex.</u> Gender differences were examined using interaction terms (sex x weight category).*
Abstract: Conclusion: First, could the authors state in their conclusion that obesity was associated with higher likelihood for sick leave and long-term absenteeism in women (not just that there was an association).	Thank you for your comment. We added it: *Our findings stress the longitudinal association between excess weight and <u>increased likelihood of</u> sick leave days as well as long-term absenteeism in women*
Abstract: Secondly, actually this study has not studied if weight management would be beneficial in reducing sickness absence. Maybe it would be safer to either exclude the last sentence from the conclusion or to conclude that obese women might need special efforts in aiming to reduce sickness absence at work.	Thank you for your comment. We removed it: ~~*Weight management strategies might also be beneficial to reduce sickness absence.*~~
Strengths and limitations of the study	Thank you for your comment. We removed it:

- could a person expert in statistics check the following sentences: “Panel regression models were used, diminishing the problem of unobserved heterogeneity.” and “Attrition bias might be rather small in the current study.” (This study uses a panel data in which some of the individuals have taken part all of the surveys and some only parts of the surveys. I think the statement is right for the first sentence but I wonder if attrition bias is small in this study.)	• ~~*Attrition bias might be rather small in the current study.*~~
In common - I would recommend the authors to use term sex instead of gender throughout the text.	Thank you for your comment. Following your suggestion, we changed it throughout the manuscript. However, we would like to keep the term “gender differences” unchanged. Please see: http://www.med.monash.edu.au/gendermed/difference.html for further details.
For the readability, please define overweight, obesity etc in the beginning of the instruction and then use the	Thank you for your comment. We changed it accordingly: *Behind the Americas, Europe ranks second regarding the proportion of*

categories without numbers systematically.	*overweight (25 kg/m² ≤ BMI < 30 kg/m²) or obese (BMI ≥ 30 kg/m²) people, according to the WHO statistics.*
Even longitudinal studies (using panel data or not) can define "causality" between the factors. Please use term "associations" throughout the text.	Thank you for your comment. We replaced terms implying causality referring to our study with "longitudinal association" when appropriate. For example (page 7): *We used fixed effects (FE) regression models to estimate the longitudinal association between excess weight and sickness absence.*
Introduction - Please, add a reference to a sentence (page 4, rows 44-46)	Thank you for your comment. We added two references to this sentence: *While some studies found evidence of an elevated risk of sick leave for pre-obese subject, others reported no significant association when compared to normal-weight subjects.[7,8]*
- Page 5: "This knowledge is important, as effective interventions to treat excess weight might also be fruitful to reduce sickness absence". How do this study actually analyze the effectiveness of interventions? I think you could rather state that association of obesity in sickness absence is important for implementing actions to tackle the problem. Further, the association of sex difference in the association is	Thank you for your comment. Following your suggestion, we changed it accordingly and moved it to the discussion section (please see reviewer #1, comment #2): *Knowledge regarding the longitudinal association between obesity and sickness absence (and the moderating role of sex) is important for implementing strategies to tackle this problem.*

important.	
Study population and methods - In general this section is clear and well written.	Thank you very much.
- Maybe I have missed it but some of the participants have taken part in all of the surveys and some occasionally? It would be informative to write this down it the methods.	Thank you for your comment. We added these details to the sensitivity analysis section: *Since the results might be affected by attrition bias, sensitivity analyses were conducted to test the robustness of our findings. We re-estimated our models on a sample including only those individuals who were surveyed in each of the six waves (27,592 observations with sick leave days as outcome measure; 6,139 observations with long-term absenteeism as outcome measure)*.
Are those included in the sick leave data also included in long-term absenteeism or have you stratified by the length of the sick leave?	Thank you for your comment. We added two sentences to the results section: *It might be the case that individuals with within-variation on sick leave days also provide within-information on long-term absenteeism. However, it is not necessarily the case.*
- Statistical analyses: Would it be more simple just to stratify the analyses by sex (as you have done) rather than report everything for all, by sex and then even with an interaction term included? I suggest the author the report that they tested the interaction	Thank you for your comment. Please see our reply to your comment starting with “Abstract: Methods: I would like the authors to mention that […]”. Following your suggestion, we also removed this sentence: *Moreover, regression analysis showed that transitions from normal weight to overweight were associated with an increase in the probability of long-*

between sex and BMI classes for sickness absence. The interactions were significant and therefore they have stratified their analyses by sex (justifying why the stratification by sex is done). Then stop telling that the interaction was significant between the sexes in “every other sentences”. Results - Related to my earlier comments, I suggest that all analyses should be stratified by sex and also reported by sex -> changes to the text of results and tables needs to done.	*term absenteeism in women, but not in men. ~~Gender differences were significant.~~*
- In general, authors are repeating the tables in their text. I think the authors are trying to help a reader to follow their tables by adding what information they are describing in which columns but this makes the text rather heavy to read. For example, the authors could exclude from the	*Results of Poisson FE regressions with sick leave days as outcome measure are displayed in Table 2 ~~(first column: total sample; second column: men; third column: women; fourth column: total sample with interaction terms (weight categories x sex))~~.* *Results of conditional FE logistic regressions (outcome measure: long-term absenteeism) are depicted in Table 3. ~~In the first column, FE regressions for the total sample was presented. In the second and third column, FE regressions stratified by sex was presented. In the fourth column, interaction terms (weight categories x sex) were added to the regression~~*

"Regression analyses" sentence from the page 10 rows 7-10 (first column….) and exclude same kinds of sentences throughout the result section	~~*model.*~~
"depicted" -> described? (page 8, row 19)	*Pooled sample characteristics for individuals included in FE regression analysis with sick leave days (column 1) and long-term absenteeism (column 2) as outcome variables are described in Table 1.* *Results of conditional FE logistic regressions (outcome measure: long-term absenteeism) are described in Table 3.*
Could you add the total number of observation also to table 1 (under sick leave days and long-term absenteeism)?	Thank you for your comment. We changed it accordingly. Please see Table 1 for further details.
In sum (page 8, row 32 ->) are you talking in total or at baseline?	Thank you for your comment. We clarified it: *In total (Table 1, columns 1 and 2), nearly one-half were female (47.8% in the sample with sick leave days as outcome, 48.7% in the sample with long-term absenteeism as outcome).*
Tables 2 and 3, exclude columns 1 and 4, add information from the foot note (Poisson …. or Odds Ration…) to the headings to help	Thank you for your comment. We changed it accordingly. Please see Table 2 and 3 for further details.

readability of the tables.	
I also wonder if it would be more informative to report 95% CIs instead of SEs?	Thank you for your comment. We changed it accordingly. Please see Table 2 for further details.
? Exclude information related to parenthesis from the headings (first column…..etc).	Thank you for your comment. We changed it accordingly. Please see Table 2 and 3 for further details.
Exclude information related to interaction terms from the tables and add this information to the results or the methods.	Thank you for your comment. We would like to remain this point unchanged. In our opinion, tables should be readable without referring to the methods section. This might ease the understanding.
Sensitivity analyses were very clear and nicely written, thank you for it.	Thank you very much.
Discussion In general the discussion was logic and informative.	Thank you very much.
I missed some deeper discussion about sex difference related to sick leave. I wonder if the psychological/ psychosocial factors explain the difference. What kinds of factors happen in a woman body when her fat level increases? Are they more	Thank you for your comment. Following your suggestion, we extended the discussion section: *Overweight and obesity have been proposed to exert a negative effect on one's body image and self-esteem and this tends to be more pronounced in women, as they may be more affected by the slim ideal compared to men.*[29] [30] *In addition, perceived weight might play a role in the relationship between weight and sickness absence, insofar as negative weight perceptions may lead to higher levels of dissatisfaction and psychological distress,*

prone to musculoskeletal diseases, diabetes or depression? Is obesity a proxy of something else?	*specifically in women.[31] Furthermore, overweight and obese women are more often targets of weight stigmatization, weight discrimination and prejudice (e.g., laziness, less self-control, work refusal), in particular regarding the workplace setting.[32-34] This may lead to higher risk of feelings of stress thereby reducing job resources and increasing job strain. Consequently, they may be more likely to employ poor coping strategies (e.g., escaping or avoiding distressing situations) which could eventually result in withdrawal behaviors such as sick leave.[28 31]* *Another explanation might be that medical consequences (e.g., musculoskeletal diseases, cardiovascular diseases or diabetes) of obesity differ to some extent between women and men[35 36]. Ultimately, these differences in morbidity might lead to differences in sickness absence between women and men. However, future research is needed to investigate this relationship.*
Please, add a reference(s) to page 15 row 31.	Thank you for your comment. We added two references to this sentence: *In total, results of this longitudinal study add to evidence from previous correlational studies, which suggest that obesity is associated with long-term absenteeism cross-sectionally[7 8].*
Page 15 paragraph 4: you mean in Germany?	Thank you for your comment. We clarified it: *Because in Germany sick pay is shortened after six weeks and not paid any longer by the employer but by a third-party payer (e.g., health insurance), and a different medical certificate has to be provided, it is expected that employees will quite accurately remember their sick leave spells. Hence, this indicator should be less prone to measurement error.*

Please exclude the first sentence.	Thank you for your comment. Following your suggestion, we excluded the first sentence: ~~*An ethical approval was not obtained because criteria for the need of an ethical statement were not met (risk for the respondents, lack of information about the aims of the study, examination of patients). However,*~~ *The German Council of Science and Humanities (Wissenschaftsrat) evaluated the German Socio-Economic Panel (GSOEP) at the Deutsches Institut für Wirtschaftsforschung, (DIW), Berlin. The German Council of Science and Humanities approved the GSOEP. The GSOEP is in accordance with the Helsinki Declaration as revised in 2008.* Again, thank you for your helpful comments. It helps to improve the quality of the manuscript.
Reviewer #3	
Since the research was a longitudinal study, using 6 repeated (biannual) measurements from 2002 to 2012. The authors used fixed methods that should be not complete. I strongly suggest to use mixed methods. There are many software packages (including freeware) for this kind of statistics.	Thank you for your comment. We clarified our choice in further detail in the methods section: *Our main goal was to provide consistent estimates under very weak assumptions (Brüderl & Ludwig, 2015, p. 352-353; Cameron & Trivedi, 2005, p. 699). Therefore, FE regressions were used. The FE specification was also preferred based on the Hausman test. For example, the Hausman test statistic was X^2=838.31, p<.001 (with sick leave days as outcome measure).* We acknowledge that other panel data methods (such as mixed methods)

	exist offering the possibility to include both time-constant and time-varying independent variables. However, further assumptions have to be made (Wooldridge, 2002, p. 266). In addition, the strengths and limitations of the FE model are also discussed by Brüderl and Ludwig (2015, p. 352-354). Brüderl, J., & Ludwig, V. (2015). Fixed-effects panel regression. In C. Wolf (Ed.), The Sage handbook of regression analysis and causal inference (pp. 327-357). Los Angeles: SAGE. Cameron AC, Trivedi PK (2005). Microeconometrics: methods and applications. New York: Cambridge University Press. Wooldridge, J. M. (2002). Econometric analysis of cross section and panel data. Cambridge, Mass.: MIT press.

VERSION 2 – REVIEW

REVIEWER	Hung-Yi Chuang Kaohsiung Medical University, Taiwan
REVIEW RETURNED	05-Feb-2018

GENERAL COMMENTS	Because the authors stated as "Because our main goal was to provide consistent estimates under very weak assumptions (cf. Brüderl & Ludwig, 2015, p. 352-353; Cameron & Trivedi, 2005, p. 699), we used FE regressions. The FE specification was also preferred based on the Hausman test. For example, the Hausman test statistic was X^2=838.31, p<.001 (with sick leave days as outcome measure). " Since I am not a statistician, I strongly suggest to find a qualified statistician to review the use of only Fixed effect in this study was appropriate or not.

REVIEWER	Maarit Piirtola University of Helsinki
REVIEW RETURNED	05-Feb-2018

GENERAL COMMENTS	Deat authors & BMJ Open Editorial This paper has improved and is in good shape now. It needs only very minor changes of which can be checked by the editorial office.

	In tables 2 & 3 titles: write "95% Confidence Interval (95% CI)" Table 3: There are some + marks in the subscripts. Should they be * ? Replace please page 14, sensitivity analyses. Please give beta with 95% CIs + p-values in the parenthesis for both women and men (not p-values for interaction in men)

REVIEWER	Laura Serra Saurina Center for Research in Occupational Health (CiSAL) Universitat Pompeu Fabra (UPF), Barcelona IMIM (Hospital del Mar Reseach Institute), Barcelona
REVIEW RETURNED	16-Feb-2018

GENERAL COMMENTS	First of all I would like to thank the authors for the accurate review they have made of the manuscript. In my opinion they have addressed most of the main points of the revision and now the manuscript is ready for the readership of the BMJ. I would also like to thank the comments and suggestions of the other reviewers who have made possible a significant improvement of the discussion I understand that the limitations of the study can not be addressed in their entirety, however, now they are well justified and argued by the authors.

VERSION 2 – AUTHOR RESPONSE

RESPONSES TO COMMENTS OF REVIEWERS

Comments of reviewers	Response (citations from manuscript printed in italics, changes are underlined)
Editorial Advisory Board	
This is an interesting and important study that is generally well conducted.	Thank you very much.
I agree that longitudinal are generally better than cross-sectional for causality (page 5), but there is still the potential for reverse causality. This is	Following your suggestion, we added a bullet point addressing reverse causality: *The possibility of reverse causality cannot be dismissed.*

mentioned briefly in the discussion (page 17) but I wonder if it needs more attention, perhaps as a one of the "Strengths and limitations of this study" bullet points. It is easy to imagine that for some people an absence of work could lead to increased weight gain, especially those who use active transport to get to work.	
It took me a while to understand the regression models, mostly because my experience with fixed effects models it outside of panel data, and therefore I did not immediately understand the need for conditional logistic regression or why time-fixed variables could not be estimated. To avoid confusion for other readers I think it would be useful to repeat the phrase "panel data" near the model description.	We added it in the statistical analysis section: *To analyze the longitudinal association between excess weight and the binary outcome long-term absenteeism, we employed a conditional logit fixed effects model, which is a common method for panel data analysis.*
I would also add what the matching variable was in the conditional logistic regression,	These regressions are commonly used for case-control studies as well as repeated measures (panel data). In our study, we have panel data. Thus, changes within units over time were examined in conditional FE logistic

presumably the participants. It might be useful to write out the regression equations to be explicit.	regressions. In this case, the units are **individuals** (also known as matched group variable in 'clogit'). We made this more explicit in the statistical analysis section: *FE models solely exploit changes within units (here: participants) over time ("within variation").*
"The outcome measure was not significantly associated with marital status" (page 11). Statements of true/false associations based purely on statistical significance are usually unhelpful and it's best to focus on the size of the association (see, for example, the recent paper by Szucs and Ioannidis, Pubmed ID= 28824397). In this case the 95% confidence interval for marital status for women (-0.02 to 0.27) includes the mean effect of obesity of 0.24. So if the obesity effect is considered important, then the 95% CI for the marital effect does not rule out an important effect of marital status. Similarly, although the interaction effect	We extended the discussion section accordingly: *Based on a nationally representative sample (GSOEP), the aim of the present study was to examine the longitudinal association between obesity and sickness absence in women and in men. Knowledge regarding the longitudinal association between obesity and sickness absence (and the moderating role of sex) is important for implementing strategies to tackle this problem. Data were taken from 2002 to 2012. Adjusting for potential confounders, Poisson FE regression analysis showed that transitions from normal weight to obesity were associated with an increase in sick leave days in women, but not in men (with significant gender differences). Moreover, regression analysis showed that transitions from normal weight to overweight were associated with an increase in the probability of long-term absenteeism in women, but not in men.* *According to previous work translating relative effect sizes (e.g., IRR and OR) into indices of effect size in public health studies,[28 29] the IRRs and the ORs found in our analyses are classified as small. However, changes in weight from normal weight to overweight were associated with an increase in odds of long-term absenteeism of more than 40 percent among women.*

between gender and obesity is "significant" there is no mention of whether the size of this interaction has any public health significance. There is no discussion of whether the size of any effects have public health significance.	
Minor comments - Give the odds ratio and 95% CI for men in the abstract so that we can compare the size of the association with women.	Thank you for your comment. We added it: *Moreover, conditional FE logistic regressions showed that transitions from normal weight to overweight were associated with an increase in the probability of long-term absenteeism in women (overweight, OR: 1.41, 95% CI: 1.08-1.85) but not in men (overweight, OR: 0.84, 95% CI: 0.65-1.09).*
- page 6 "survey attrition is low", it would be useful to quantify this, less than 10% per wave?	Thank you for your comment. We added more details: *In addition, it was found that survey attrition is low in the GSOEP (in most years and sub-samples, attrition was less than 10% (Kroh et al., 2018)).*
- It was good to see the VIF checked. Were the residuals checked too? This can be useful for multimodality, outliers, etc.	Thank you for your comment. We also performed a residual analysis, but no obvious model inadequacies were found.
- "Confounders" used in abstract and results, but "covariates" used in methods.	Thank you for your comment. To be consistent “covariates” was replaced with “confounders” throughout

	the manuscript. For example: *Several sociodemographic, health-related and subjective well-being factors that have been identified by prior research to be associated with both excess weight and productivity loss, or proposed to influence the relationship between obesity and sickness absence were entered as potential confounders in the analyses.*
Also, why is sex listed as a confounder if it can't be used in the model because it is time-invariant?	Thank you for your comment. We clarified it by adding the following sentence in the methods section: *Moreover, the time-invariant variable sex was used for descriptive purposes.*
- Table 2, the phrase "Poisson coefficients" is not familiar to me. Presumably the table shows log relative risks. These could be exponentiated to give relative risks that may make it easier to interpret the public health significance, although for the interaction effects the estimates need to be given for specific combinations.	Thank you for your comment. In Table 2, Poisson coefficients were replaced by incidence rate ratios. We also replaced it throughout the manuscript. For example: *Adjusting for potential confounders, regressions showed that transitions from normal weight to obesity were associated with an increase in the probability of sick leave days in women (incidence rate ratio (IRR): 1.27, 95% CI: 1.02-1.57), but not in men (IRR: 0.85, 95% CI: 0.68-1.06).*
- "results not shown, but available upon request", why not just add them to an online	Thank you for this suggestion. We provide an online appendix with these additional results.

appendix?	
Reviewer #1	
First of all I would like to thank the authors for the accurate review they have made of the manuscript. In my opinion they have addressed most of the main points of the revision and now the manuscript is ready for the readership of the BMJ. I would also like to thank the comments and suggestions of the other reviewers who have made possible a significant improvement of the discussion. I understand that the limitations of the study can not be addressed in their entirety, however, now they are well justified and argued by the authors.	Thank you very much for reviewing this revised version of our manuscript.
Reviewer #2	
Deat authors & BMJ Open Editorial This paper has improved and	Thank you very much for reviewing this revised version of our manuscript.

is in good shape now.	
In tables 2 & 3 titles: write "95% Confidence Interval (95% CI)"	Thank you for your comment. We corrected it.
Table 3: There are some + marks in the subscripts. Should they be * ? Replace please	Thank you for your comment. In the Table legend we indicated that "*": p<0.05 "$^{+}$": p<0.10 In order to avoid misinterpretations, the "+ marks" were superscripted.
page 14, sensitivity analyses. Please give beta with 95% CIs + p-values in the parenthesis for both women and men (not p-values for interaction in men)	Thank you for your comment. We added these values: *In addition, regressions showed that transitions from normal weight to overweight were associated with an increase in the probability of sick leave days in women (IRR: 1.19, 95% CI: 1.01-1.41), but not in men (IRR: 0.91, 95% CI: 0.75-1.10; with significant interaction term, p<.05).* Following the suggestion made by the editor, Poisson coefficients were replaced by incidence rate ratios and 95% CI were reported.
Reviewer #3	
Because the authors stated as "Because our main goal was to provide consistent estimates under very weak assumptions	Thank you for your comment. The last author is an empirical microeconomist, specialized in the area of

(cf. Brüderl & Ludwig, 2015, p. 352-353; Cameron & Trivedi, 2005, p. 699), we used FE regressions. The FE specification was also preferred based on the Hausman test. For example, the Hausman test statistic was X^2=838.31, p<.001 (with sick leave days as outcome measure)." Since I am not a statistician, I strongly suggest to find a qualified statistician to review the use of only Fixed effect in this study was appropriate or not.	econometrics dealing with panel data. We also talked with experts in panel data econometrics (political scientists, sociologists, and statisticians) about the statistical approach chosen in this manuscript. They strongly endorsed the strategy used.